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THE POSTURE OF THE HEAD IN ACCIDENTS WHEN THE PATIENT IS UNDER AN ANÆSTHETIC.

By H. A. Hare, M. D., *Professor of Therapeutics in the Jefferson Medical College of Philadelphia.*

[*Read before the Johns Hopkins Hospital Medical Society, November 19th*, 1894.]

In the presence of an accident from an anæsthetic the physician at once resorts to artificial respiration, after administering circulatory stimulants, and carries out his object by resorting to one of the several methods generally recommended for this purpose.

Be this method what it may, some studies which have been made with Dr. Edward Martin lead me to believe that it is of little value if the posture of the patient's head and neck is not correct, since the positions naturally assumed by the head of the patient at such times are generally capable of making all efforts at artificial respiration difficult or impossible.

As long ago as 1889, Howard, of London, published a very interesting paper on this topic which has since been widely quoted. While recognizing the value of his studies, my own have led me to reach somewhat different conclusions in regard to the posture of the head and its influence on the patulousness of the windpipe, and it is to these studies that I ask your attention. Howard's statements in regard to the rôle of the epiglottis in cases of arrested respiration in anæsthesia are as follows:

1. The epiglottis falls backward in apnœa and closes the glottis; therefore the first thing in order and importance is the elevation of the epiglottis.

2. Traction upon the tongue, however, whatever the force employed, does not and cannot raise the epiglottis, as supposed.

3. The epiglottis can only be raised by extension of the head and neck.

The question which naturally arises first, is Howard correct in regarding the epiglottis as the cause of the obstruction? Personally, I believe he is wrong, because in the great majority of cases the air passages are at once cleared of obstruction simply by drawing the tongue forward, a method resorted to by all of us, yet one which, as Howard himself states, and as we have proved, has absolutely no effect on the epiglottis unless the traction is applied well back on the dorsum of the tongue by a tenaculum. We may conclude, therefore, that the epiglottis is not the chief cause of the obstruction and that the tongue is more frequently at fault, but as any obstruction is undesirable, and as the epiglottis does sometimes certainly partially close the windpipe, what shall be done to govern its position? Howard states that this may be accomplished solely by the posture of the head. The method which he recommends is as follows:

"Having, by bringing the patient to the edge of the table or bed, or by elevation of the chest, provided that the head may swing quite free, with one hand under the chin and the other on the vertex, steadily but firmly carry the head backward and downward; the neck will share the motion, which must be continued till the utmost possible extension of both head and neck is obtained. Sometimes a slight elevation and extension of the chin will at once check stertor or irregularity

of breathing; but understand, the extension, which can in no case do harm, should always be rather more than appears necessary. It should never be forgotten, however, that the full effects of extension as above described can be secured with certainty only by making the extension complete as directed."

Once more the studies which I have made of this subject have convinced me that Howard's advice is not practically valuable. Although there can be no doubt that the changes described are produced, so far as the position of the epiglottis is concerned, on the other hand such a position of the head and neck as he directs has the effect of strapping the soft palate over the dorsum of the tongue, thereby cutting off the entrance of air through the mouth and renders the nostrils the only path for its entrance. As the nasal cavities are in many persons obstructed by exostoses, hypertrophies or polyps, the nostrils do not afford a sufficiently certain entrance space for air, and removal of glottic closure by this posture may cut off the air higher up.

If, on the other hand, the head is extended and simultaneously projected forward, both the tongue and epiglottis are raised and the soft palate is so drawn as to permit of free breathing through the mouth as well as the nose. This is shown in the specimen which I now show you, in which the basilar process of the occipital bone is chipped away and the naso-pharynx exposed.

Returning to the question of the various modes of performing artificial respiration, such as Sylvester's or Marshall Hall's, let us see what accurate measurements of the volume of air pumped into the chest show as to their relative value. To determine this point the respiratory tract was connected with an ordinary gas meter, properly adjusted by means of a two-way tube, through one valve of which the air entered readily, while it could only escape through the meter. Curare was used to prevent voluntary breathing. When the Sylvester method was used the quantity of air passing out of the chest equalled 62; when that of Marshall Hall was employed the quantity was represented by 22. In another experiment the Sylvester method gave 18, while the Marshall Hall gave 8. It is evident, therefore, that the Sylvester is actually, as we have long believed it to be, by far the best method. In this connection it was found that in Sylvester's method it is vitally important to have an assistant grasp the feet and hold them motionless, since in this way the extension and upward traction of the arms above the head elevates and dilates the chest. This particularly is the case in children and persons of small weight, as the lower segment of the body readily follows the chest in its upward movement.

Very closely connected with the questions first considered is the condition of the respiration, so far as its nervous control is concerned, in accidents from chloroform and in shock and cerebral concussion. The position of the medical profession is at present uncertain in regard to the dominant action of chloroform, chiefly because of the contradictory views expressed by special students of its powers, and the teaching of certain leading therapeutists and surgeons whose opinions are radically different. Further than this, many experimental investigations have seemed to reach quite different results and have apparently left the subject more clouded than ever. Aside from the question, long since settled, that chloroform is the more dangerous anæsthetic in its immediate effects, we may without difficulty reconcile nearly all the contradictory results so far obtained if the individual researches are carefully studied, and as a result of such reconciliation reach the absolute conclusions so necessary in so important a subject. The conclusions are as follows, namely, that after its primary effect on the vaso-motor system, the dominant action of chloroform is certainly upon the respiratory centres in the medulla, and that this effect is the cause of death in most cases of chloroform accident. Not only does nearly all experimental work teach us this, but in a collective investigation made by me some time since as to the cause of death in man under chloroform, nearly every case reported was found to have suffered primarily from respiratory arrest. These statements are based first upon the report made by myself and my assistant, Dr. Thornton, to the Hyderabad government in India, and upon the confirmatory but entirely independent studies of Randall and Cerna recently completed in Texas, in which these investigators took up the study to prove that our studies were erroneous and were forced to admit that death is due to respiratory failure.

Believing then that death is generally due to this cause when chloroform is given, it is incumbent upon the anæsthetizer to watch the respirations, both because death creeps on in this way, and also because the rapidity and depth of breathing governs the dose of the drug, for the dose is not the amount poured on the inhaler but the amount taken in vapor into the chest. Lawrie's assertion that chloroform should be given only while the respirations are regular and withdrawn as soon as they are stormy is most wise.

While I believe the respiratory action to be the dominant one in producing death as a rule, no one who has studied the effects of chloroform can deny that death may occur under its influence, in cases which are diseased, by its cardiac effect. Any shock may kill a case of cardiac disease, and it is natural therefore that any drug which possesses the peculiar influence of chloroform over the heart may be prone to cause death in this way.

In other words, supposing that the amount of depression from very full doses of chloroform equals 25 units, this amounts to little in the normal heart; but if the heart be depressed 25 additional units by disease, the depression of 50 units may be fatal, particularly if to this 50 is added 25 units more of depression through fright and cardiac engorgement, through disordered respiration or struggling. That true depression of the heart-muscle may take place under chloroform seems to us most undoubted, as we think that the tracings in every research that we have seen support this view. There is always a decrease in cardiac power manifested by the decrease in the force of the individual pulse-beat, and this passes away only if chloroform is removed early enough. We also agree with McWilliams that from the very first inhalation of chloroform there is a constant tendency to cardiac dilatation.

Closely associated with influence of chloroform on the vital functions is its influence upon the blood-vessels, which, as

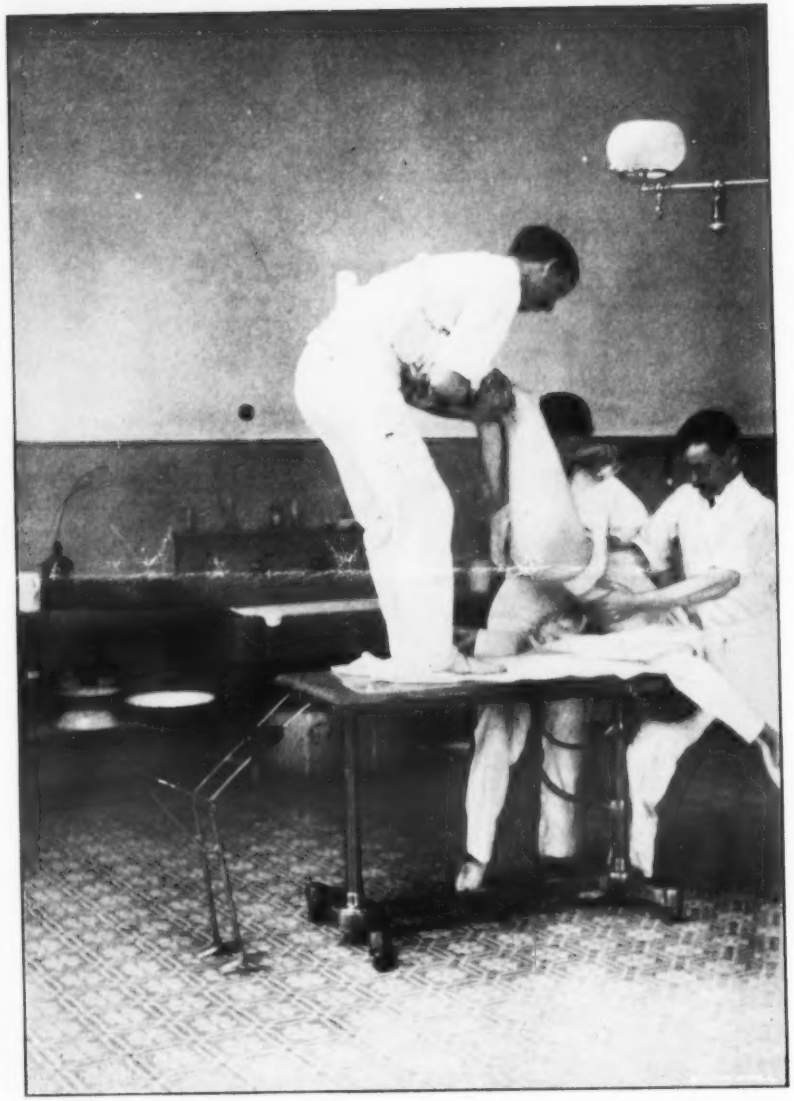

FIG. I. INSPIRATORY MOVEMEMTS.

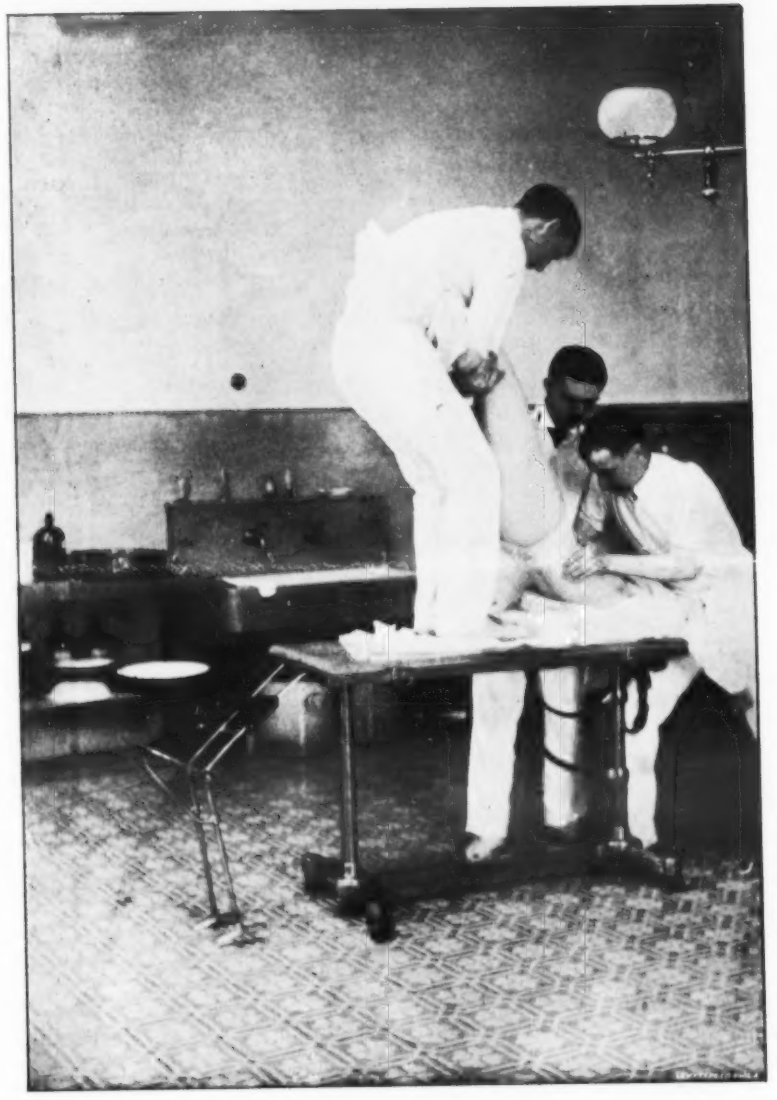

FIG. II.—EXPIRATORY MOVEMENTS.

already stated, is its primary and dominant effect. This influence I believe to be very much more worthy of attention than is generally recognized. Every physiologist knows that the action of the heart and respiration is greatly influenced by vaso-motor relaxation. The gasping respiration of sudden faintness is probably due more to sudden vascular dilatation than to direct failure of the heart, and the exceedingly rapid pulse of shock is seen in conjunction with the relaxed blood-vessels so characteristic of this state. The integrity of the vaso-motor system is as necessary to life as the integrity of the heart, since it is under the government of this system that the cardiac mechanism is active and the vital interchanges take place throughout the body. Acting upon this belief I have found both in the laboratory and at the bedside that atropine enables more chloroform to be given without circulatory depression than can be used if no atropine is administered, and there is good reason to believe that the use of atropine by surgeons for the purpose of stimulating the respiratory function, or preventing cardiac inhibition by irritation of the vagus, in reality prevents dangerous symptoms chiefly by its vaso-motor influence.

For some months I have been interested in studying the condition of the respiration in cases of traumatic shock, and it is surprising to note how death comes from failure of this function in distinction from failing circulation. Further than this, the employment of artificial respiration in these cases will often save life.

Very recently, in cerebral concussion, Horsley has called attention to these facts and has practiced artificial respiration with good results in apparently hopeless cases.

DISCUSSION.

DR. KELLY.—We give chloroform frequently in the Gynæcological Department, and, although in a very dangerous atmosphere, I also gave it in Philadelphia a great many times, before coming to Baltimore, but always in dread, because Dr. Wood of the University of Pennsylvania had said that any surgeon having a death from chloroform should be indicted for murder. The main reason why Philadelphia surgeons are afraid of chloroform is because they do not know how to give it. In abdominal surgery chloroform is better than ether, as it gives a quiet anæsthesia, rapidly produced, and its after-effects are not so disagreeable. My personal preference, save in cases of grave cardiac complications, as a dilated heart, or where there is failure in compensation, is always for chloroform. As I leave the choice of the anæsthetic, however, to my anæsthetizers, I find that in a large majority of cases they select ether. I never ask an assistant to give chloroform who is averse to it, especially if he has not been accustomed to its administration. The man who administers chloroform should be afraid of his anæsthetic. He should watch his patient closely, and constant attention should be given to respiration, pulse and general appearance. Since the results of Dr. Hare's researches have been published, in which he proves that the respiration is the important factor and fails first before the heart, we pay more attention to the respiration than before.

Regarding methods of resuscitation, I have found a method of my own exceedingly satisfactory. I have treated about fifteen cases with uniform success by this method, which I believe to be the best for keeping up artificial respiration. I find too that I have been following the principle laid down by Dr. Hare—that of the extended and slightly flexed head. On the first indication of failing respiration the administration of the anæsthetic is instantly suspended, and the wound protected, if abdominal, a broad piece of gauze is laid over the intestines under the incision. An assistant steps upon the table and takes one of the patient's knees under each arm, and thus raises the body from the table until it rests upon the shoulders. The anæsthetizer in the meanwhile has brought the head to the edge of the table, where it hangs extended and slightly inclined forward. This position, shown in the accompanying cuts, is similar to that described by Dr. Hare and resembles that taken by the runner when he is breathing hard. The patient's clothing is pulled down under her armpits, completely baring the abdomen and chest. The operator, standing at the head, institutes respiratory movements as follows: inspiration by placing the open hands on each side of the chest posteriorly over the lower ribs, and drawing the chest well forwards and outwards, holding it thus for about two seconds (Fig. I); expiration, reversing the movement by replacing the hands on the front of the chest over the lower ribs and pushing backwards and inwards, at the same time compressing the chest (Fig. II). The success of the manœuvre will be demonstrated by the audible rush of air in and out of the chest.

The heart and pulse should be constantly watched. As respiratory movements are continued, a little flickering pulse-wave will be observed at the wrist, which shortly becomes faint and regular, and gradually increases in strength. From ten to thirty of these acts of induced respirations will usually suffice to excite voluntary respiratory movements, which begin with short, jerky, gasping breaths, becoming louder and then regular. The movements must then be timed to suit the natural efforts. As the depth of inspiration increases, the color slowly returns, the pupils contract, and the danger is past. In women with contracted, fusiform chests (tight lacers), this procedure is not available; in such cases respiration should be induced by direct antero-posterior compression of the chest by placing one hand on the lower third of sternum, and the other on the back opposite the first, and alternately squeezing the chest and relaxing the pressure, when air will be audibly forced in and out, and the patient revived as by the previous method; it also fails in a rigid old chest.

The suggestion which Dr. Osler once made concerning the use of external heat during the administration of an anæsthetic in a prolonged or a severe operation is a very important one. Dr. Osler especially impressed me with this fact two years ago on his return from London, where he had seen Horsley conduct his experiments in brain surgery on monkeys which were kept on a warm table during the operations. Horsley lays especial stress on keeping up the body temperature, to prevent shock. Following this suggestion, I have recently had narrow hot water-bags made three feet long, which we keep in the operating room, and in case the operation is to be prolonged, or the patient is feeble, we place one on either side of her

body and an ordinary water-bag at the feet. I am indebted to Dr. Hare for several important hints, especially concerning the use of atropine in cases of disturbed respiration. I am also glad that he has placed the principle of the proper position of the head upon a scientific basis.

DR. OSLER.—With reference to the position which Dr. Kelly puts the patient into, I will mention the very interesting experiments made in Dr. Sanderson's laboratory in Oxford by one of his assistants upon the influence of position on blood pressure. With a very carefully adjusted turn-table, the blood pressure was found to rise immediately as the lower extremities of the animal were raised.

DR. HALSTED.—I am pleased to hear what Dr. Hare has said, and I am sure that the position of the head which he advocates is the correct one. It is the position which we always use. We have learned to use it from experience. Dr. Hare said "Now you have got the position," when I was testing on the cadaver our position in order to see whether or not it opened the glottis. In pulling the jaw forward as we do it, one necessarily extends the head. In anæsthetizing a patient we always catch the jaw close to the condyle and press it as strongly forward as possible, and so keep the glottis open. If this is properly done it will never be necessary to pull the tongue forward with an instrument. It is not, therefore, the extending of the head which opens the glottis. If we were to extend the head by pulling the ears we should not open the glottis. The extension of the head is simply incidental to the drawing forward of the jaw. I agree with Dr. Hare when he suggests that we might make use of atropia oftener than we do. It is a drug upon which we can rely to increase arterial tension. But morphia is a vaso-motor depressant and lowers arterial tension; hence I do not use it in conjunction with ether. I am afraid of chloroform and do not use it. In Germany, where they certainly ought to know how to give it, where they use it almost exclusively and write a great deal about the proper method of administering it—giving it drop by drop, a drop with each inspiration—they have had more deaths this year than ever before from chloroform, 1 to 1600 or 1700, according to Gurlt's statistics. For the last ten or twelve years Gurlt has, as you know, gathered statistics from the different German universities. The usual mortality is 1 to 2200 or 2300. This year from every university in Germany, almost without exception, the mortality from chloroform has been greater than for many years. That is very remarkable unless the manufacturers of chloroform are to blame. One death should be enough to deter a man from ever using it again. Dr. Lange took Dr. Kelly's attitude for a good many years, then he had a death on the table and said that he would never give chloroform again. It is perhaps possible to give morphia in so small a dose that it may for a few moments act as a vaso-motor stimulant and increase the arterial pressure, but in moderate and particularly in large doses it lowers arterial tension most pronouncedly. These statements are supported by the highest authorities,* and I take pleasure in calling Dr. Hare's attention to them.

DR. HARE.—A characteristic symptom of the first stage of opium poisoning is a slow, full and strong pulse, and therefore the arterial pressure must be high.*

There are one or two points raised in the discussion that I would like to speak of.

I thoroughly agree with Dr. Kelly, although I am one of the much maligned Philadelphians in this instance, when he says that many persons don't know how to give chloroform in Philadelphia. In two of the cases in which I have seen accidents occur, the chloroform was given very much more as if it was ether than if it were chloroform; and in the last case I saw, after the woman was once resuscitated, the resident physician two minutes later pulled the napkin over the patient's mouth and poured on about ½ ounce of chloroform so that her pulse was lost at the wrist and her breathing stopped a second time.

In regard to atropine, I think we do not use large enough doses of this drug. When I was a student a proper dose of atropine was 1-250 gr. and of strychnine 1-160. Now some surgeons give as much as ½ gr. of strychnia, and atropine in the dose of 1-100 to 1-50. Atropine is a better drug than we think it is, and does not get the credit it ought to have, simply because we do not give it in large enough doses. One one-hundredth of a grain would be a very proper dose, and I have given myself, in cases in which I had reason to believe there was a condition of vaso-motor relaxation, very much larger doses, proportionately, than this. In a child of 8 months I have given 1-150 gr. of atropine twice in 8 minutes, and I believe that it saved the child's life.

This leads me to emphasize one other point which I am almost afraid to speak of, because I have emphasized it so often, particularly to the students of Jefferson College: I am confident that we let many cases die on account of vaso-motor relaxation. When you see the diagrams in the books on physiology, of the enormous area of the vascular system when relaxed and the capacity of it as compared with the arteries and veins, and when you read of the influence of vaso-motor relaxation in producing tachycardia and cardiac exhaustion, then you can appreciate the importance of the vaso-motor system in maintaining life. In pneumonia, when you have a very feeble and very rapid heart, don't think that because the heart is rapid digitalis should be given. It is extraordinary the way the action of the heart will improve just as soon as you develop the normal resistance of the vascular system. The heart working against a relaxed vascular system is in a worse condition than when working against a vascular spasm such as we have in chronic nephritis.

*C. BINZ: Ueber den arteriellen Druck bei Morphium-Vergiftung. Deutsche med. Wochenschr. 1879 and 1880.

FICK: Ueber die Blutdruckschwankungen im Herzventrikel bei Morphiniumnarcose. Verhandlungen des Cong. für innere Med. 1886.

BINZ: Discussion of Fick's paper. "We have learned to-day from Dr. Fick, by reason of his exceedingly precise methods of investigation, that morphia in doses which are not large weakens the heart's systole, and therein lies a fresh proof of the old experience that morphia is a heart poison of such power that it may endanger the central organ of the circulation."

HARNACK: Arzneimittellehre und Arzneiverordnungslehre, p. 650.

* DR. HALSTED.—With the slowing of the pulse the fall in arterial pressure increases. *Vid.* Binz, Heubach, Fick and others.

Since Dr. Halsted has fired a shot at the therapeutist, I will have to have a shot at the surgeons in return. Dr. Abel will agree with me when I state that morphia is not a vaso-motor depressant; on the contrary, it is recognized as quite a powerful stimulant to the heart and vaso-motor system in ordinary doses.

The position I want to emphasize about the head under these circumstances is that it should be extended and craned forward in order to let the air pass in. One of the deaths I saw, which occurred in Dr. Goodell's practice, was due, I am confident, to respiratory failure. I took charge of the head and Dr. J. Wm. White used Sylvester's method of artificial respiration, and it was interesting to notice the difference in the respiratory sounds when the head was in the ordinary position and when it was pushed forward in the way I have described.

The last point perhaps may have some relation to the question which Dr. Halsted has brought up in regard to why it is that in some years there are more deaths than in others. So far as I know, there have been no carefully carried out experiments in regard to the fatality of chloroform under varying conditions of the atmosphere. In Galveston there are very few deaths from chloroform. Lawrie has now had about 30,000 chloroform anæsthesias without a death, and only a few accidents, not alarming. Perhaps it is that the condition of the temperature of the air—humidity and barometric pressure may have something to do with the quantity of chloroform which is taken into the chest, for it is not the quantity of chloroform that is put on the towel, but the quantity of chloroform which the patient takes into his lungs from the towel that is to be considered. This emphasizes still further Lawrie's statement that just as soon as the patient's respiration is getting stormy we must stop the administration, because if you do not do so you will not know how much chloroform the patient is getting.

DR. ABEL.—The question is a matter of dosage. Small or therapeutic doses of morphine have no effect to speak of on blood pressure. It is a very different matter when toxic doses have been taken. It is a notable fact that morphine has a more powerful action on the respiratory centre than on the vaso-motor centres.

I have listened with great pleasure to Dr. Hare's interesting paper. His report to Lieutenant-colonel Lawrie, of the Hyderabad commission, to which he has made reference in his remarks to-night, contains valuable confirmatory researches on the effect of chloroform on the respiration and circulation, and all of us, I feel sure, will agree with him in his conclusions on these points.

The question has been raised to-night of the relative value of ether and chloroform. I was myself brought up under a chloroform regime, and when I first began to teach I put it rather more highly in the list of anæsthetics than I am inclined to do to-day. The Germans are now making a careful examination of the comparative merits of chloroform and ether. Many of their surgeons who have hitherto favored chloroform above ether are turning about and it would appear that chloroform is going to lose the day. Laboratory investigations are giving us fresh proofs of the greater safety of ether.

Chloroform has a remarkable affinity for some of the substances composing the nervous system. The brain and the medulla seem able to pack it away even when it is breathed in very dilute air solutions. Thus Kronecker and Cushing have found that the breathing of air containing only 0.34–0.42 per cent. by volume of chloroform will still lead to paralysis of the respiratory centre; and Pohl, following out some early work of Schmiedeberg's, has shown that in the stage of complete anæsthesia the brain contains about three times more chloroform than an equal weight of blood, blood containing 0.015 per cent. and brain substance 0.0418 per cent. chloroform. We have some information, therefore, as to the localization of chloroform in the body. Schmiedeberg long ago demonstrated that the serum of the blood contains very little chloroform during anæsthesia, not more than would be dissolved in water, and that the chloroform taken up and carried by the blood is bound to its red and white corpuscles. From experiments made by Pohl we know that it is the lecithine, cholesterine, fatty matters, and the protagon of the corpuscles of the blood and of the cells and fibres of the central nervous system to which the chloroform is tied. What proportion of chloroform is taken up by such viscera as the liver we do not yet know.

If so weak a solution as 0.5 per cent. of air volume will still, after being breathed for some hours, cause cessation of breathing, that is rather against chloroform, even in the light of modern improvements in its administration. Paul Bert in 1884 proposed that only a "titrated" air solution, containing at the most no more than 4 per cent. of chloroform, should be used for anæsthetic purposes. This method was employed for a time by a few practical anæsthetists (Clover) and is said to have reduced the number of chloroform accidents. But for some reason or other, either because accidents still occurred or because the required apparatus was cumbersome, the method was given up.

The method of *le mélange titré* is, however, being revived, only ether is being used instead of chloroform. Dr. Spenzer, an American chemist, working in Schmiedeberg's laboratory, has found that the inhalation of air containing 1.5 per cent. of ether by volume, for two hours, causes no anæsthesia in animals, the result being only a mildly hypnotic condition. If the air breathed contained 2.5 per cent. of ether by volume the anæsthesia was also found to be entirely incomplete, the reflexes in this instance being exceedingly lively. When the respired air contained 3.19–3.62 per cent. of ether, complete anæsthesia was attained within 25 minutes, and could be kept up for hours without any respiratory disturbance whatever, and without damage to the heart. When 4.45 per cent. by volume of ether was employed, anæsthesia was complete within fifteen minutes, the breathing was slower but regular, the heart-beats a little more rapid and weaker than normally, but still of a regular rhythm. At 6 per cent. by volume of ether admixture the limit of safety was reached, for now cessation of the respiration occurred within 8–10 minutes after allowing the ether to be breathed. Artificial respiration, however, always restored the animal, no matter how often the experiment was repeated. Spenzer's experiments, in which careful chemical analyses of the respired air

were made, thus substantiate Snow's results, gained many years ago (1858) by crude methods, that an air mixture containing about 3.5 per cent. by volume of ether will keep up an anæsthesia for many hours without endangering the respiration or circulation. We see, too, from Spenzer's experiments that even with ether there is but a narrow limit between safety and death.

This summer, in the pharmacological laboratory at Bonn, I saw an apparatus which allows us to give ether to human beings according to this safer method of *le mélange titré* with great success. The arrangement is such that the mixture can be made of air and ether in such proportions as you want it. It is then carried through valves that are so easily moved that there is no work of any consequence for the chest of the patient. I was told there that they had anæsthetized patients by this method with great success. A further advantage of this method is that air mixtures of ether that do not much exceed 4 per cent. are non-irritating to the mucous membrane of the respiratory tract. Dreser has made experiments on this point and has found that up to 5.4 per cent. by volume of ether vapor the mixtures were pronounced by his subjects to be easily borne.

The absence of knowledge as to toxic doses, that is, as to the amounts pro kg. of body weight from which no recovery is possible, is greatly to be regretted.

Unfortunately, too, we have, as far as I am aware, no careful experiments as to the localization of ether in the organs of the body, and no equally exhaustive experiments as to its chemical fate in the organism as compared with chloroform.

One other point in the way of chloroform is the tendency to degeneration of the important viscera after its use. You cannot chloroform the healthiest and strongest bulldog, notably a tough species of animal, two successive times without his dying from the after-effects of the drug. Keep him deeply under anæsthesia with chloroform for four hours, let him recover, and on the third day following repeat the experiment, keeping him again deeply under the influence of chloroform for four hours, put him away into his pen, he will regain consciousness as usual, but in the course of a few days death will ensue. Whether carefully wrapping up an animal in cotton wool would keep the animal alive after two such periods of heavy chloroforming I cannot yet say.

I have been much struck by the poisonous nature of chloroform in feeding experiments that I have undertaken for the purpose of studying changes in the metabolism of the liver. Repeated doses, even when not large, and single large doses (6–9 grams) according to the weight of the animal, soon cause a profoundly cachectic condition, the animal's coat becomes shaggy, it loses weight and in the course of 6–14 days it dies, no matter how healthy it was before. Others have demonstrated that in such cases a marked fatty degeneration of many organs, notably of the liver, kidneys and heart, has been induced. Kast and Mester have come to the conclusion after an examination of the urinary constituents, particularly of the so-called "neutral" sulphur compounds, that long-continued chloroform inhalation induces a profound disturbance in proteid metabolism, extending over several days. Of no little importance is the fact that it requires considerable time for the organism to get rid of chloroform, whether taken up from the lungs or from the digestive tract. This is demonstrated by the increased elimination of chlorides after chloroform anæsthesia, experiments on animals showing that the greater part of the chlorine of the retained chloroform is excreted in the form of chlorides, and that even on the fourth day after the administration of the chloroform the urine still contains an excess of chlorides.

One reason then why the Germans are turning about is that their studies have led them to believe that many of the deaths that occur very shortly after a prolonged administration of chloroform are due to the serious lesions of important organs induced by this drug. Virchow's Archiv and other journals and the inaugural dissertations of the last seven or eight years have had numerous contributions on this subject.

A recent contribution by Selbach from the laboratory of Prof. Binz, entitled "Are fatal after-effects to be feared as resulting from long-continued ether inhalations?" reviews the literature on the untoward effects of chloroform, and describes a series of original experiments made with the view of determining the poisonous after-effects of prolonged etherization, and from these the author is led to infer that there is little or no danger of a fatal after-action following anæsthesia by ether in the case of human beings. Here too, then, on this important side of the question the advantage lies with ether.

Dr. Theobald.—Regarding the use of atropine preceding the administration of chloroform, I may say that for a number of years I have been in the habit of giving a hypodermic injection of ¼ gr. morphine with $\frac{1}{150}$ of atropine previous to the use of chloroform, and the effect has been most satisfactory. Not only is the heart-depression in a great measure obviated, but the patient comes more quietly under the anæsthetic, and the recovery from the anæsthesia is slower and more satisfactory. The patient does not wake up suddenly with restlessness, but wakes up in a sleepy, good-natured state, and submits to the dressing with less objection.

THE JOHNS HOPKINS HOSPITAL REPORTS.

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Contents: The Results of Operations for the Cure of Cancer of the Breast, performed at the Johns Hopkins Hospital from June, 1889, to January, 1894.

By WM. S. HALSTED, M. D., *Professor of Surgery, Johns Hopkins University, and Surgeon-in-Chief to the Johns Hopkins Hospital.*

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A CONTRIBUTION TO THE STUDY OF ANÆSTHESIA BY ETHER.*

BY PROF. EXTRAORD. H. DRESER, *of the Pharmacological Laboratory at the University of Bonn.*

INTRODUCTORY NOTE BY PROF. ABEL.

Prof. Dreser has forwarded me the following paper in English for publication in this country. While his researches into the composition of the air in the closed masks described by him might appear to have less value for those that administer ether by the open method, yet a close reading of his exhaustive paper will prove of value to all anæsthetists alike. That a deficiency of oxygen can so easily be induced in a closed mask, and that there is more danger on this score than from the accumulation of carbon dioxide, are points that are well worth establishing. It must also be of great interest to know that mixtures of air and ether containing more than 7 per cent. by volume of ether vapor cannot be inhaled without great irritation to the mucous membranes of the respiratory tract. Prof. Dreser has devised most ingenious apparatus which makes it possible to anæsthetize patients with mixtures of air and ether of known strength. In a trial experiment, recently made in the gynæcological clinic of Prof. Fritsch,† at Bonn, a woman was kept under the influence of ether during an hour and twenty-one minutes, the time required for the performance of an operation for prolapse of the uterus, the anæsthesia being entirely satisfactory to the operators. Anæsthesia was first induced with a mixture containing 6 per cent. of ether, the mixture was then increased to 8 per cent. of ether until profound anæsthesia was attained, and then lowered to 4 per cent., at which strength it was maintained throughout the entire operation. All this was accomplished without depriving the air in the mask of its oxygen and without allowing the expired air to accumulate, the mask used containing two very simple and very mobile valves which separate the air to be inspired from that expired. It is to be hoped that Prof. Dreser's method of administering ether in the form of titrated mixtures will be found so practicable that anæsthetists will have no difficulty in employing it. The greater safety of such a method and its advantages in lessening the untoward after-effects of anæsthesia are evident.

JOHN J. ABEL.

Even the healthiest person may by some accident, as a fracture, a dislocation or a wound, become a subject for the surgeon, who is then obliged to anæsthetize him in order to come up to the motto of his profession, to cure *tuto, cito et jucunde.*

The anxious feelings which a patient who may perchance know something of these matters must have before the beginning of an operation, will be increased by his suspicions as to whether either Julliard's or Wanscher's mask may be quite fit to prevent any accident occurring. This matter is therefore not only of scientific value, but also of a personal interest. Such were the suggestions that occurred to me on the occasions when I witnessed surgical operations, and that induced me to make the analysis of the gas contained in the inner chamber of these masks.

I do not want to speak of the diverging assertions of clinical empiricism, but wish rather to furnish the precise data of analyses, in order that we may judge whether either method will prove efficacious and not dangerous.

Surgeons now try to substitute less dangerous agents, such as bromide of ethyl, ether, or pental, for chloroform, which is suspected of being a heart-poison. The vapors of these fluids, which are more volatile than chloroform, must accumulate in the air, to be inhaled in a far greater quantity than in the case of chloroform. Therefore the simple Esmarch mask used for chloroform is not efficient. The permeable cover of the Esmarch mask would bring about quite the contrary effect. The air exhaled by the patient being of a higher temperature than the surrounding atmosphere, a great quantity of ether would escape into the room; whereas the cooler atmosphere of the room, passing through the cover of the mask at the next inhalation, will carry too little ether into the patient's lungs.

For this reason the great basket-mask of Julliard has an impermeable cover of waxed taffeta, and the Wanscher mask has a bag of india rubber.

However, in spite of these differences, the principle which led to their construction is the same, viz. to have a pretty large fore-chamber close to the nose and mouth of the patient. The warm air exhaled must remain in this chamber until the next inhalation, and by parting with its store of heat causes a more rapid evaporation of the narcotizing fluid. In Wanscher's mask, if it fits the patient's face well, the exhaled air will remain entirely in the india rubber bag; in Julliard's mask it only partially remains. Thus the anæsthetic vapors will reach the lungs of the patient in sufficient quantity, perhaps more even than is required.

The question now is, whether the air thus tarrying under the mask and breathed again may not gradually be exhausted of its oxygen and overloaded with carbonic acid; and in this event these two methods of applying anæsthetics would have to be considered dangerous.

Before giving the results of my analysis of the gas met with in the masks, and the physiological conclusions to be drawn from it, it is necessary to state the methods which I used.

A. THE METHOD USED IN OBTAINING THE GASES IN THE MASKS AND IN DETERMINING THEIR VOLUME.

A small quantity of the gas (about 100 cc.) to be analyzed was drawn from the inner chamber of the mask by means of glass tubes joined together with india rubber to a conduit, thus yielding without breaking to every movement of the patient. This conduit had been previously filled with concentrated salt water in order to prevent as much as possible any absorption of the gases. The end piece of the conduit, bent

* Researches on the composition of the air in the masks of Wanscher and of Julliard during anæsthesia by ether.

† H. Dreser: Demonstration eines Apparates für Herstellung dosirter Aetherdampf-Luftmischungen. Sitzungsb. d. Niederrhein. Gesellsch. f. Natur- u. Heilkunde zu Bonn.

like a knee, was introduced between the border of the mask and one side of the nose. The estimation of the volume of the compounds was made according to Prof. Hempel's expeditive method for the technical analysis of gas.* As the solubility of ether vapor in water is not inconsiderable, one must use concentrated salt water instead of ordinary water, or else the percentage of the ether vapors would fall short of that actually present, especially when the analysis is not done quickly.

I have tested in the following way the possibility of transferring ether containing air into Hempel's burette with sufficient accuracy by using concentrated salt solution as a separating fluid. Fifty cc. of air were measured into Hempel's burette and propelled into a vessel filled with salt solution saturated with ether. On shaking the air in this vessel, its volume was increased by the ether evaporating from this salt solution. In order to prove how much the volume in the shaking vessel might have diminished after carrying it back into Hempel's burette and measuring its new volume, the lower end of the shaking vessel was joined to a 100 cc. pipette by a rubber tube. After shaking, the pipette was filled with salt solution to its upper mark, placed on the same level as the fluid in the shaking vessel. Having again transferred the gas into Hempel's burette and noted its volume, the remainder of the 100 cc. salt solution in the pipette was weighed, and by means of the specific gravity of the salt solution the volume of air increased by the vapors of ether was calculated. The difference between the measured and the calculated values of the gas volume may be shown by the following table:

	Read off.	Calculated.
I	64.0 cc.	64.25 cc.
II	80.2	80.02
III	69.7	69.85
IV	86.8	86.97
V	62.4	62.35
VI	67.8	68.02

Hence it is clear that the difference is about 0.2 cc. when using salt solution as a separating fluid.

A few explanatory words about the absorption of the narcotizing vapors in the analysis must be given. Hempel's experiments in 1891† proved that the vapors of carburetted hydrogen are readily absorbed by absolute alcohol. I made experiments proving the absorption of ether and bromide of ethyl by means of Hempel's "Aethylenpipette"‡ filled with absolute alcohol, using, however, the precaution of not letting the alcohol rise up to the caoutchouc of the connecting capillary tube. The gas to be analyzed was repeatedly propelled and carried back until two equal volumes were noted. Thus I have proved that any further absorption is impossible; but the volume of the remaining gas was still too great, because it contained some alcohol vapors. These vapors are easily removed by tilting the closed burette and moistening its walls with the separating fluid. The efficacy of this method can be tested by introducing 100 cc. of pure air into the alcohol pipette. The air carried back into the burette is increased by the alcohol vapors to somewhat more than 100 cc.; after tilting, the original 100 cc. will be found again.

TEST FOR THE ANALYSIS OF ETHER AND BROMIDE OF ETHYL.

	ETHER.			BROMIDE OF ETHYL.		
	Air.	Air increased by the vapors.	Air free from vapors.	Air.	Air increased by the vapors.	Air free from vapors.
I	50.0 cc.	59.8 cc.	50.2 cc.	50.0 cc.	59.2 cc.	50.0 cc.
II	"	56.8	50.1	"	56.6	50.2
III	"	65.4	50.2	"	58.6	50.1
IV	"	60.2	50.0	"	56.0	50.2
V	"	63.8	50.2	"	56.6	50.0
VI	"	62.2	50.2	"	64.4	50.3

B. OBSERVATIONS ON ANÆSTHESIA BY BROMIDE OF ETHYL BY MEANS OF WANSCHER'S MASK.

The following 24 observations were made during *shorter surgical observations.* The Wanscher's mask used had a sponge near the metallic mouthpiece of the mask, upon which the liquid was poured. The free border of the mouthpiece was formed by an india rubber roll inflated with air in order to make it fit tightly to the patient's face. Nevertheless the mask did not always fit quite closely, because many a patient struggled vigorously at the very beginning. This may be one reason for the somewhat different percentages found in the proofs of air taken out of the mask.

No.	BrC_2H_5 vol. per ct.	CO_2 per ct.	O_2 per ct.	Remarks.
I	8.0	2.2	12.4	Proof taken after 1-1½ minutes.
II	8.2	3.0	10.6	" " " " "
III	4.2	12.0	7.1	" " " 3-4 "
IV	6.8	2.2	14.6	" " " 1-1½ "
V	4.0	2.2	16.2	" " " 30-40 seconds.
VI	6.2	3.3	13.0	
VII	5.4	2.4	14.7	
VIII	2.4	1.6	18.4	" " " 20 seconds.
IX	5.0	2.2	14.6	
X	6.8	2.2	13.0	" " " 55 seconds to 1 min.
XI	7.7	2.9	13.2	
XII	4.0	2.2	16.4	" " " 30-40 seconds.
XIII	12.0	2.6	11.6	" " " " " " struggling much.
XIV	4.0	3.2	13.4	" " " 2 minutes.
XV	7.2	3.8	10.6	" " " 1 minute, struggling.
XVI	9.6	2.9	11.8	" " " 1½-2 minutes.
XVII	10.6	2.6	12.6	" " " 35-45 seconds.
XVIII	12.8	2.8	7.4	" " " " " " struggling violently.
XIX	7.2	2.8	12.8	" " " 50-55 seconds.
XX	7.8	2.2	12.8	" " " 55 seconds.
XXI	2.4	2.6	16.4	" " " 1 min.-1 min. 10 secs.
XXII	13.8	1.8	12.8	" " " 80 seconds.
XXIII	10.0	2.8	10.4	
XXIV	14.6	1.9	13.1	" " " 1 minute.

The limits were 2.4–14.6 per cent. of bromide of ethyl, 2–3 per cent. of carbonic acid (with one single exception); the differences were mostly found in the percentages of oxygen, varying between 7.1–18.4 per cent.; the average was 12–14 per cent. O_2. It was necessary to extend these

*Gasanalytische Methoden von Walther Hempel. 2te Aufl. 1890.

†W. Hempel und G. M. Dennis: Über die volumetrische Bestimmung der dampförmigen Kohlenwasserstoffe. Ber. d. deutsch. chem. Ges. zu Berlin, 1891, p. 1162.

‡Hempel: Gasanalyt. Methoden, p. 182.

C. INQUIRIES TO HEALTHY PERSONS WITHOUT NARCOTIZING THEM.

This furnished me with a much-wished-for opportunity to compare the feelings of conscious individuals breathing under the mask, concerning the percentage of the air met with in the inner chamber of it. The experiments were performed on men sitting perfectly quiet. The proofs of gas were taken after breathing half a minute, one minute, and two minutes. We had the following results:

After half a minute.

Dr. A. K.		St. K.		H. D.	
CO_2 per ct.	O_2 per ct.	CO_2 per ct.	O_2 per ct.	CO_2 per ct.	O_2 per ct.
5.8	13.6	5.3	13.9	5.0	15.4
5.6	13.8	5.5	14.3	6.2	13.6
6 2	13.5	5.8	14.0	6.6	12.3
6.0	13.9	4.8	15.4	7.6	11.8
5.4	14.2	5.4	14.4	6.0	14.2
5.6	14.0	5.6	14.6	6.3	12.9

After one minute.

7.2	9.7	6.5	12.5	6.6	11.6
6.4	10.6	5.6	13.6	6.8	10.6
6.2	11.2	6.2	12.6	6.2	12.6
6.9	10.1	6.2	12.8	6.6	11.8
7.0	9.8	6.8	11.2	7.6	9.8
7.0	9.8	6.6	12.2	7.4	10.6

After two minutes.

6.2	9.6	7.8	8.8	6.4	10.4
7.2	6.4	7.8	9.4	6.8	10.6
7.6	6.0	7.7	7.1	7.0	8.8
7.8	**5.0**	8.8	**5.8**	8.2	**6.0**
7.0	8.0	8.0	6.6	7.8	7.4
6.0	10.8	8.2	6.4	7.0	9.5

The differences in these experiments are not quite so considerable as those met with in the narcosis by bromide of ethyl, yet they were greater than was expected. The reason was the different volume of the air extant in the india rubber bag, which varied according to the more or less deep folds of it.

In the experiments with bromide of ethyl, the air in the mask seemed to be especially impaired by loss of oxygen when the patients had been struggling. On that account I engaged a student to take exercise with dumb-bells in order to study the influence of muscular exertion on the composition of the air in the bag. The dumb-bells used weighed 12 kilos, the height to which they were lifted up being 1.25 meters; the work done with each lift was 15 kilogrammeters. There were 8–9 lifts in half a minute; the work done in this time was 120–135 kilogrammeters, and 240–270 kilogrammeters during one minute. When sitting perfectly quiet the following composition was found:

After half a minute.		*After one minute.*	
CO_2 = 4.4 per cent.	O_2 = 16.0 per cent.	CO_2 = 5.8 per cent.	O_2 = 13.4 per cent.
5.9	14.4	5.6	13.2
6.2	13.8	6.9	11.9

When working, was found:

After half a minute.		*After one minute.*	
CO_2 = 8.2 per cent.	O_2 = 10.6 per cent.	CO_2 = 8.0 per cent.	O_2 = 9.4 per cent.
7.9	9.7	8.9	11.0
7.2	11.4	7.0	10.4
6.2	12.5	6.6	11.4
6.8	11.0	7.4	10.0
		8.2	7.6

The results of all these experiments without narcotics show that the oxygen contained in the bag of the mask decreases so rapidly that after having breathed for half a minute in the mask, the light of a candle is extinguished in this air. I found that the percentage composition of air in which a candle had gone out was 16.2–15.4 per cent. O_2 and 3.6–4.6 per cent. CO_2. Cl. Bernard* has obtained corresponding numbers, viz. 15.4 per cent. O_2 and 2.3 per cent. CO_2. After having breathed for one minute in Wanscher's mask, the partial pressure of oxygen had sunk to one-half of its pressure in the atmosphere —a limit when bad symptoms also began to appear in the well-known experiments of Paul Bert. After having breathed in the mask for two minutes only, 5–6 per cent. of oxygen were several times met with. The consequence was that very disagreeable oppression of the heart and violent dyspnœa were caused, so that the persons experimented upon were glad when the two minutes, the time of the experiment, were over. By comparing each single experiment we have evidence that it is not the percentage of carbonic acid, but only the diminished oxygen, which is the cause of this state of suffocation combined with cyanosis of the face or slight dizziness. In the last experiment on the influence of muscular activity with only 7.6 per cent. O_2, violent dyspnœa was especially complained of. It is therefore clear that a partially narcotized patient who struggles to get rid of this dangerous state will make his condition rapidly worse by his muscular straining. By way of comparison with the various percentages found by me, I quote Cl. Bernard's experiments upon animals which died when the percentage of the air in the room had gone down to 3–5 per cent. O_2; in these experiments the exhaled carbonic acid was absorbed. In similar experiments of W. Müller the lethal limits were 1–5 per cent.; Stroganow found 3–4 per cent. O_2; Friedländer and Herter 3.8–2.1 per cent. The explicit researches of these latter investigators show that the diminution of the oxygen to 12.7 per cent. caused a little dyspnœa and a slight irritation of the vasomotor centers, producing only a small rise of the arterial blood pressure; but when the air breathed had only 5.1 per cent. O_2, the blood pressure rose 43 mm. of mercury above the normal. In these experiments on animals, air containing only 7.5 per cent. O_2 called forth a decided dyspnœa.

Now it may be clearly seen the Wanscher mask offers the unwelcome possibility that these states of deficiency of oxygen, already well known by experiments upon animals, will be reproduced in man in the course of a few (1–2) minutes. Among the numerous recommendations of the Wanscher mask at least some kind of information should have been given as to how often this mask should be replenished with air. The simple test with a burning candle, well known to any intelligent workman who intends going down into a well, should have warned the advocates of Wanscher's mask.

D. ON THE IRRITATING PERCENTAGE OF ETHER VAPOR.

The recommendations of the Wanscher mask say little or nothing of the percentage of oxygen and carbonic acid, or of the percentage of ether vapors to be met with in the mask.

* Léçons sur les substances toxiques, p. 220.

These concentrations, which irritate the mucous membranes of the respiratory organs, are most important for the lungs of the patients. The frequent catarrhal affections of the bronchi and lungs met with after ether narcosis are caused by too strong concentrations of the ether vapors. In order to state the percentage of ether vapors still respirable without molestation, we experimented upon ourselves by breathing proofs of several mixtures made by diluting ordinary air with varying proportions of air saturated with ether vapor. These mixtures having been prepared in an india rubber bag, two or three persons breathed immediately one "proof," wrote down their sensations, *i. e.* whether they were "bearable," or caused some "irritation and cough," or "impossible to be breathed." Immediately afterwards a proof was taken out of the bag in order to determine the percentage of ether vapor. On the whole 18 experiments were made.

	Ether vapor. per ct.	
I	6.4	Two persons moderately irritated to cough; the third very little.
II	8.8	None of the three could breathe in this mixture; cutting sensation in the throat.
III	9.0	Same as II; contraction of the glottis.
IV	7.2	Irritation and cough; cannot be breathed over again.
V	8.6	Irrespirable.
VI	6.4	Moderately irritating; two persons, "bearable."
VII	7.4	Irritating, cough.
VIII	7.0	Irritating.
IX	3.8	Well bearable, easily respirable.
X	4.8	Bearable.
XI	3.6	Without molestation.
XII	2.6	" "
XIII	4.4	Well bearable.
XIV	5.2	Well respirable.
XV	5.4	" "
XVI	5.8	Causing only little sensation.
XVII	6.2	Without much molestation, but with moderate irritation.
XVIII	6.4	Moderately irritating; when breathed several times becomes molesting soon.

Hence the percentage of ether vapors to be breathed in a conscious state by the patient should not exceed 7 per cent., as even this concentration causes some irritation and cough. The reflex movement of cough is an unmistakable evidence that the vapors of the anæsthetic have reached the patient's lungs in too strong a concentration, and that in this way the lungs will be injured. When the patient by inhaling weaker concentrations of ether vapors has been made insensible, to such a degree at least as to show no more reflex action, this very state will favor the injurious effect of the stronger concentrations upon the lungs. As long as the patient is conscious, the reflex contraction of the glottis prevents the "irrespirable" gas or vapors from entering in the finest air passages. The physician who administers the narcotic should now take care that the lungs of the narcotized patient will not be injured. In order to obtain complete narcosis as soon as possible, it has been recommended to shake the fluid ether in the bag of the mask, whereby the concentration of ether vapors is increased to the maximum. Thus the physician himself produces these injurious concentrations instead of avoiding them. I believe that it is only by a shorter duration of such dangerous inhalations that the patients are prevented from suffering greater injury, so that they usually escape with slighter irritations of the bronchi.

The temperature of the air in the mask often amounting to 31° C. after having been breathed for one minute, favors the quicker evaporation of ether. Since none of the operators at Bonn make use of this model of Wanscher's mask for ether narcosis which I employed in the bromethyl narcosis and in my experiments upon healthy persons, I was obliged to perform a sort of artificial respiration by means of a bell-jar going up and down in warm water and propelling 500 cc. of the air into the mask to and fro. The border of the mask was closed by a caoutchouc membrane, a T tube of glass was put through the membrane, one branch of the T tube communicating with the bell-jar and the other with Hempel's burette. The temperature of the air varied in these experiments between 20° and 31° C. The percentage of ether vapors differed very much, as to the phase of respiration in which the gas proof was taken, whether the bag containing the fluid ether was shaken or not. The following table shows the percentage met with:

	Temp.	Vol. per ct. of ether vapors.	Remarks.
I	31.5°C	34.0	Strongly shaken.
II	19.2	6.2	Without shaking.
III	20.4	6.6	" "
IV	19.5	6.4	" "
V	19.8	6.8	" "
VI	19.8	7.0	" "
VII	20.4	28.6	Strongly shaken.
VIII	20.8	29.4	" "
IX	20.7	23.2	Moderately shaken.
X	21.4	31.2	Ether abundantly poured in, moderately shaken.
XI	21.8	27.8	Moderately shaken.
XII	21.4	28.4	" "
XIII	31.0	14.8	Without shaking.
XIV	31.0	15.7	" "
XV	26.0	7.4	" "
XVI	26.5	22.8	Shaken.
XVII	22.5	4.0	Without shaking; taken at the end of exhalation.
XVIII	23.7	11.6	Moderately shaken; taken at the end of an inhalation.
XIX	22.5	8.6	Without shaking, at the end of exhalation.
XX	22.3	18.4	Shaken; taken at the end of inhalation.

The results of this table show that the percentage of ether vapors differed exceedingly; the minimum was 4 per cent., the maxima were 34 per cent. and 31 per cent.; the latter exceeded the limit of 7 per cent., which could still be endured more than four times. It is impossible to regulate the quantity of ether vapor for the patient in exact proportions, and the estimation can only be made by analysis. By the kindness of Professor Fritsch, who tried the newest model of Wanscher's mask, recommended by Grossmann in some gynæcological operations, I had an opportunity to get 6 proofs of the air breathed by the patients. However, before proceeding I must mention that this newest model with its circular border placed on a straight place cannot fit the patient's face so well as the former mask, especially when it is very thin. This apparent defect proves to be salutary to the patient because it prohibits the danger of deficiency of oxygen. But even with that mask it is quite impossible to obtain a mixture of air and ether to be relied upon. The following analyses show that even when

the ether is only moderately shaken, the percentage of ether vapors very easily exceeds the irritating concentration of 7 per cent.

	Ether vapor. Per ct.	CO_2 Per ct.	O_2 Per ct.	
I	16.8	3.0	12.4	After 2 minutes, moderately shaken.
II	14.6	1.6	14.6	" 4 " lastly something shaken.
III	6.8	1.2	17.8	" ½ minute, without shaking.
IV	4.6	1.2	17.2	The mask rests upon the face very loosely.
V	10.8	1.8	16.8	Ether in the mask moderately shaken.
VI	12.8	0.9	14.4	Moderately shaken.

In a former paper* I have published the results obtained with Julliard's mask on surgical patients at Tübingen. This mask was either wrapped in a dry towel only, or it remained uncovered. The composition then found was much more gratifying than that met with in Wanscher's mask. Lately I have had an opportunity to get some analyses in the case of patients that were being narcotized with Julliard's mask, the capacity of which was something greater than that used on a former occasion; besides in all these cases the mask was wrapped in a *wet* towel, which is not so porous as a dry one. The following analyses show the effect of such modifications, which at first might appear quite unimportant:

	Ether vapor. Per ct.	CO_2 Per ct.	O_2 Per ct.	Operations.
I	13.8	1.8	14.0	Osteotomy (genu valgum).
II	9.6	1.6	15.8	Ektropium.
III	7.3	1.9	16.4	Extirpation of glands.
IV	3.6	2.4	17.2	" "
V	16.4	1.8	13.4	" "
VI	8.8	2.2	15.7	Ulcus cruris, transplantation.
VII	6.8	2.5	14.5	Amput. phalang. I digit. med.
VIII	10.6	1.8	15.4	Resectio genu.
IX	2.4	2.2	16.4	" " 25 minutes after pouring in.
X	13.6	1.7	14.9	" " 10 " " "
XI	6.6	1.4	17.0	" " 5 " " "
XII	7.3	1.9	16.2	" " 1 minute " "
XIII	9.4	1.6	14.9	Ulcus cruris.
XIV	4.9	2.3	16.8	" 5 minutes after pouring in.
XV	14.4	2.2	11.2	Scraping out a tubercul. phalanx.

The application of a wet towel produces a much higher percentage of ether vapors, *in maximo* 16.4 per cent.; however, this high percentage decreases soon after the ether has been poured in. But generally the percentage of ether vapors was shortly after this process still greater than 7 per cent., which is certainly too great for a conscious person as well as for further inhalation. After some struggling the narcosis was complete sooner than in the former manner. The average of carbonic was somewhat greater and that of oxygen less than I had found in my former researches on Julliard's mask without any towel. Nevertheless, even if a wet towel was wrapped round Julliard's mask and it had been lying for a long time (for instance 25 minutes) on the patient's face, the volumes of oxygen and carbonic acid are far less to be feared than those met with after one minute's breathing in the narcosis by bromide of ethyl with Wanscher's mask. The air in the latter has been rapidly exhausted if the mask had been lying a longer time on the patient's face. In my later experiments with Julliard's mask the inner chamber was considerably larger than that of the model formerly used. The rapidity with which the air in the inner chamber of Julliard's mask is restored by the respiration of the patient depends upon the ratio existing between the volume of each inhalation (ca. 500 cc.) and that which the prominent part of the patient's head allows to remain in the inner chamber.

In order to determine this volume, the mask put under water was emptied into a measuring cylinder. The mask formerly used contained 1400 cc., the new one 2100 cc. The volume of the patient's head, surrounded by the border of the two models of the mask, was about the same. It was fixed in the following way: The border of the mask was marked on the skin of the face with an aniline pencil; then the person dipped her face up to the pencil-mark into a vessel previously filled with water up to its very brim; the displaced water caught in a salver gave the volume of the covered part of the head, amounting to 780 cc. Consequently the air remaining in the former mask was 1400 cc.—780 cc. = 620 cc.; in the newer mask we had 2100 cc.—780 cc. = 1320 cc. Every inhalation will restore 500 cc. in the inner chamber of the masks; in the former mask the patient has restored $\frac{500}{620}$ cc. = 80 per cent. of the air by each breath; in the mask used latterly he restored but $\frac{500}{1320}$ cc. = 37.8 per cent. The wet towel has the effect of increasing still these 1320 cc.

With one single exception the percentage of carbonic acid in the C_2H_5Br narcoses was never great enough to cause it to remain in the system. We know from the researches of Mr. G. Strasburg, made by means of the ærotonometer in the laboratory of Prof. Pflüger, that the carbonic acid in the venous blood of the heart has a partial pressure on an average 5.4 per cent. of the atmospheric pressure. The partial pressure of carbonic acid in the arterial blood is equal to only 2.8 per cent. of the atmosphere. In the experiments on narcotized patients the percentage of CO_2 met with was, as a rule, far below 5.4 per cent., which means that the discharge of carbonic acid from the blood into the air of the lungs is continually going on, although it is somewhat *protracted* in proportion with the diminished fall of partial pressure. In the experiments without narcotics the tension of carbonic acid frequently exceeds 5.4 per cent. after having been breathed for half a minute in Wanscher's mask; when working only for one minute even 8.9 per cent. CO_2 was found in one experiment. It is very likely that when the anæsthetic vapors are breathed the exhalation of carbonic acid is somewhat diminished in comparison with the normal state.

When Julliard's mask has no towel wrapped round, the percentage of carbonic acid was only 1.7–1.2 per cent. One per cent. of carbonic acid is not at all dangerous, for the workmen digging the Gotthard tunnel could work hard in air containing 1 per cent. of carbonic acid for several hours without injurious consequences.

Some authors assume the narcosis with Julliard's mask to be a narcosis of ether mixed with carbonic acid which could not be exhaled when using that mask. It is very easy to refute such an assertion by simply comparing the few minutes

* Ueb. d. Zusammensetzung des bei der Aethernarkose geatmeten Luftgemenges. Beitr. z. klin. Chir., X., p. 412.

which are necessary to produce ether narcosis, and the time which would be required for an individual to produce the quantity of carbonic acid necessary to narcotize himself. The data for this simple calculation we find in a treatise of W. Müller,* published in 1858 under the direction of Prof. C. Ludwig. These experiments showed that carbonic acid administered in a proper dose acts as a narcotic poison, and that it is able to kill an animal in a comparatively short time. In order to produce narcosis in an animal it is necessary that the quantity of CO_2 which its body is to absorb amounts to the third part, and to produce death to half of its own volume (0.567–0.584). To calculate the time necessary for such an auto-intoxication, we must consider that a person lying quietly on the operating table will not produce more than 5 cc. CO_2 per kilogram of bodily weight in one minute. The volume of a kilo is about a liter = 1000 cc.; a kilo of man must therefore produce and retain the third part of 1000 cc.; the time required for producing these 333 cc. would be $\frac{333}{5} = 66.5$ minutes. During the few minutes in which an ether narcosis is complete it is impossible to accumulate a quantity of carbonic acid of any consequence, especially when the greater part of this gas has been exhaled, at least when using Julliard's mask.

In Friedländer and Herter's experiments,† the partial pressure of carbonic acid had to amount to at least 25 per cent. of atmosphere in order to produce in 1–2 hours a state of narcosis, but still quite insufficient for surgical purposes. The percentage of carbonic acid met with in my experiments, even on healthy persons, was *never high enough* to produce the slightest narcosis. On the contrary, according to the explanations of Prof. Miescher,* the lower percentages inhaled, as 3–12 per cent. of CO_2, strengthen the breathing movements; 10–12 per cent. cause decided dyspnœa with deep inhalations and active exhalations. Therefore the percentage of carbonic acid met with in the air of the masks cannot at all be looked upon as having a paralyzing or narcotizing effect.

By comparing the results of my former analysis with those given in the preceding pages it is obvious that among the methods of etherization used at present, the method of Julliard without any towel must as yet be considered the most favorable. The volume of air, which the covered part of the patient's head allows to remain in the inner chamber, should not exceed 600 cc.

The desirable end to be attained in narcotizing is to prepare and keep well-known proportions of ether vapor and air well regulated and constant; this is as important in the administering of anæsthetics as the prescribed doses of medicine taken internally. Just as the maximal dose of morphia and other strong acting drugs is fixed in the German pharmacopœia, so should the vapors of the volatile poisons, such as our anæsthetics (chloroform, ether, bromide of ethyl, etc.), not exceed a maximum percentage when they are administered.

*Beiträge zur Theorie der Respiration. Sitzgsber. d. Wiener Akademie, 1858. XXXIII. Bd., p. 99.

†Ueber die Wirkung der Kohlensäure auf den thierischen Organismus von C. Friedländer u. E. Herter. Zeitschrift für physiolog. Chemie, II., 99.

*Bemerkungen zur Lehre von den Athembewegungen. Archiv für Anat. u. Physiologie, Physiolog. Abtheilg., 1885, p. 368.

CATHETERIZATION OF THE URETERS IN THE MALE.

By James Brown, M. D., *Assistant in Genito-Urinary Surgery.*

[*Read before the Johns Hopkins Medical Society, December* 17, 1894.]

Catheterism of the ureters, which has for some time past been so frequently resorted to in the female to determine the limits of disease in the upper urinary tract, will doubtless in the near future be as frequently practised in the male, since we hope to show you to-night it can now be done as readily in the latter as in the former. Since the method employed by us involves the use of the Nitze-Leiter cystoscope, a few words respecting the construction of this instrument may not be out of place.

In 1887 two instruments, constructed upon the same principle, made their appearance almost at the same time—one by Nitze, made by Hartwig of Berlin, and one from Leiter of Vienna; these two gentlemen, who had been associated in the construction of the platinum loop cystoscope of 1879, having quarreled and separated. We will first describe the Leiter instrument. This has the shape of a short beaked sound. Two forms are made: one, known as the anterior, for the examination of the anterior surface, vertex, neck and sides of the bladder; and one for the base and posterior surface, called the posterior cystoscope. They are alike in outward form, and differ merely in the position of the light and the window. Each is composed of three parts—beak, shaft, and ocular end. The beak, which contains the small incandescent lamp, consists of a hollow metal hood with a long oval aperture covered in by a solid piece of rock crystal. This opening for the exit of the rays of light is placed on the anterior or posterior surface of the beak according to the kind of instrument. The hood can easily be screwed off and on to allow of access to the little lamp. The terminals of the lamp fit into two sockets, and are brought by means of insulated surfaces in direct communication with the battery. In the concavity of the elbow of the anterior instrument is placed a window prism, to refract the rays of light from the object looked at on to the end of the telescope. In the posterior instrument the window is at the convexity of the elbow, and is simply covered in by a plane glass window, as the object observed is in a direct line with the observer's eye. The shaft has two compartments; one serves for the reception of the telescope tube, and one, a very small tube, contains the insulated wire for connecting the lamp with the battery, the circuit being completed by the wall of the

instrument. The telescope tube, which extends from the ocular end to the elbow, is removable and can be used with either instrument. The ocular end has an arrangement for connecting the battery wires—a switch or key for opening and shutting the circuit, and a small knob on its rim which serves to indicate the position of the beak, and thus enables us to know what part of the bladder we have under examination. The only notable difference between the Leiter and Nitze cystoscopes is in the arrangement of the beak. In the Nitze instrument the tip of the beak is in the form of a hollow silver cap which is provided with a small oval aperture for the exit of the rays of light. The aperture is only covered in with the thin glass of the lamp, which is firmly cemented into the cap. Nitze claims that, owing to the great resistance of these small lamps when properly made, the pane of rock crystal is superfluous and has the disadvantage of obstructing the rays of light as well as of compelling us to use a smaller lamp.

In December, 1888, Dr. Brenner of Vienna had Leiter to place along the under surface of the shaft of the posterior cystoscope a small canula, whereby the fluid in the bladder could be changed without removing the cystoscope. This canula, which is 2 mm. in diameter and incorporated with the shaft of the instrument, terminates just below the window at the vesical end, while externally it is prolonged with a curve downwards separately from the shaft for a distance of several cm. Dr. Brenner afterwards attempted by passing a catheter through this canula to catheterize the ureters. He was successful in one female case, but failed in the male.

Others, it seems, made similar attempts. Thus Mr. E. Hurry Fenwick, in his excellent book on "The Electric Illumination of the Bladder and Urethra," in speaking of this instrument, gives his opinion upon it as follows: "I have had Brenner's pattern, and I believe Mr. Harrison has also used it; it has been returned by both of us as unpractical. As regards its adaptability for catheterization of the ureters *of the female* I have nothing against it. The orifices of the ureters will be rarely found, however, so patulous or so well placed as to allow of such a proceeding being accomplished by means of this instrument."

Now it is this same instrument we employ and find little or no difficulty in catheterizing the ureters in the male or female. So far we have not a single failure to record.

Respecting the mode of its performance this is very simple. With the bladder containing if possible from 150 to 200, or even as much as 300 cc. of fluid (for the amount that will cause the ureteral orifices to present most favorably varies in different cases), we pass the anterior cystoscope and take a complete survey of the bladder. This done, we replace it with the Brenner instrument, which is passed with its stylet in. The ureteral orifices are searched for; these being found, the stylet is removed, catheter inserted and passed nearly to the inner opening of the canula, ureteral orifice is again found and the catheter is passed into it. To prevent kinking of the catheter and to guard against exerting undue traction upon the ureteral orifice, the cystoscope must be kept in line with the catheter as long as the latter is within the ureter. Not infrequently we have found it of great advantage to give the catheter a slight curve at its tip. Such a curve enables us, by rotating the part of the catheter external to the bladder between the thumb and finger, to vary the direction of its tip in the bladder. If a small wire, to which has previously been given the desired curve slightly exaggerated, be passed into and left in the catheter when not in use, it will be found that the curve thus given the tip of the catheter will be retained when the wire is withdrawn. This is always done before introducing the catheter into the canula. Not infrequently this little manœuvre has enabled us to overcome the faulty presentation of the ureteral orifice of which Mr. Fenwick speaks.

In considering the advisability of an operation upon one of the kidneys and the kind of operation that had best be performed, if any, one would be largely influenced in his decision by the conclusion he arrived at respecting the second kidney. As one would not think of performing nephrectomy in bilateral suppurative or tubercular disease, so one would not remove a kidney, even though the seat of a neoplasm, if he was convinced either that it was the only kidney or of the incapacity of its fellow to carry on the necessary renal function. It has been generally recognized by surgeons that the best possible way to determine the condition of the second kidney would be to collect its secretion unmixed with that of its diseased companion. While in the female, thanks to the efforts of Pawlik, Simon, Kelly and others, catheterization of the ureters is generally recognized as the only means of reaching this end with any degree of certainty; in the male, prior to the introduction of Brenner's modification of the Nitze-Leiter cystoscope, such a procedure had been so commonly regarded as impracticable that almost no effort had been made in this direction. The various methods proposed for compressing one ureter and thereby obtaining the unmixed secretion of the opposite side have not been reliable in their results.

The brief histories of the following three cases serve to illustrate both the value of catheterization of the ureters as a diagnostic means and the class of cases to which it is applicable.

Case 1.—Mr. T. was a patient in this hospital under the care of Dr. Osler and was referred to us last May for catheterization of the ureters. He was 19 years old and gave a history of frequent and at times severe paroxysms of pain in the right loin, dating back four years. The first attack followed two days after falling against a bar, striking upon his right side. It continued for two days and then disappeared, leaving him free for a month. He then had a second attack of a similar character. The pain was sometimes behind, sometimes in front, but never radiated downwards. As time went on the attacks became more and more frequent and occasionally were attended by nausea and vomiting. The urine was noticed on one or two occasions to be bloody. In 1892 he contracted syphilis and gonorrhœa. On May 12, 1894, both ureters were catheterized and from 3 to 4 cc. of urine collected from each. The specimen from the right side was cloudy and the microscope showed it to contain numerous polynuclear leucocytes. That from the left was also cloudy, but it became perfectly clear on the addition of a minute quantity of acetic acid. Microscopically it was found to be almost normal, containing merely a few red and white blood-cells, doubtless attributable to the catheter. He was transferred from the medical to the surgical side, and on

May 23d the right kidney was removed by Dr. Halsted. A stone was found occupying one of the infundibula. After recovering from the operation, the urine, except for the presence of a few threads in the first glass, the remnant of the attack of gonorrhœa, was found to be perfectly normal.

Case 2 was a gentleman from St. Louis, brought to me, September 19, 1894, by Dr. Black, of that city. Mr. S., married, æt. 62. His history is as follows: In 1873, after lifting a heavy weight, he was seized with a severe pain in the left renal region. It came on suddenly, and after lasting a day or two it gradually disappeared. It was seated just below the false ribs and radiated downwards to the testicle, penis and thigh. It was accompanied by nausea, vomiting and diarrhœa and a frequent desire to micturate, the urine being bloody. A few weeks subsequently he had a second attack, but on this occasion the pain was suddenly relieved, a small flattened calculus the size of a large pea being soon afterwards discharged. Subsequently he had two other attacks, one in 1875 and one in 1886. From this time on up to six months ago he had suffered from a constant dull pain in the left renal region and numbness of the corresponding thigh. At times he would experience sharp cutting pains in the left loin, but these would last only a few minutes. Violent or sudden movements he instinctively avoided. Up to five weeks ago he had been almost entirely free from pain, and says that he felt better than he ever had since his first attack. All along his general health has been good, his weight averaging 171 pounds.

Status præsens.—Complexion sallow, well nourished; weight 171 pounds, height 5 feet 11 inches, micturition normal, suffers from a constant dull pain in the left loin, together with a feeling of numbness along the outer part of the left thigh. Appetite is moderately good, tongue clean and moist and bowels regular, heart and lungs normal, arteries soft and corneæ clear. Neither kidney is palpable. Pressure over left kidney elicits pain more marked behind than in front. Pressure along the corresponding ureter is also painful, especially at the point where it crosses the brim of the pelvis. No pain on pressure over corresponding points of the opposite side, nor in the hypogastrium. Both testicles were found hard and atrophied, and over each there was a depressed scar. Meatus small, 20 F., while a stricture was found 3 inches long in the pendulous urethra, commencing one-half inch from the orifice. The meatus being cut up to 29 F., and the stricture not admitting of the passage of the cystoscope, gradual dilatation of the stricture was advised to be practised until a 29 F. sound could be passed into the bladder. A specimen of urine which the patient had just passed previous to visit was of normal color, acid reaction, specific gravity 1021, and slightly opaque, a small amount of yellowish sediment being deposited on standing. It contained no sugar, but was highly albuminous. The microscope showed the sediment to consist of pus and epithelial cells, the latter of various shapes, together with hyaline and granular casts. In the early part of November Dr. Black returned with his patient, stating that the stricture had readily yielded to the dilatation and a 29 F. sound could be easily introduced. On the 21st of November, with the patient under chloroform, after having made several fruitless attempts without general anæsthesia, both ureters were readily catheterized and several cc. of urine obtained, the catheter being passed several inches up the canal. These specimens, together with a specimen of the mixed urine, were submitted for examination to Dr. Lewellys F. Barker, at the Anatomical Laboratory, who kindly wrote out the following report:

Report on specimens of urine sent by Dr. Brown, November 21, 1894, Mr. S.

The *mixed urine* is of a pale straw color, turbid, and deposits on standing a whitish, flocculent but not abundant sediment; contains a small amount of albumen, no sugar, no bile; yields no diazo-reaction of Ehrlich; on microscopic examination there are a few hyaline and finely granular casts, a number of pus corpuscles with polymorphous nuclei, a few small mononuclear cells, numerous squamous epithelial cells, and also red blood corpuscles to be seen. The specific gravity is 1012. The reaction of the urine is acid.

The *urine from the left ureter* also contains a few granular and hyaline casts, red blood corpuscles, epithelial cells and many pus corpuscles. It contains a small quantity of albumen, but very much more than an equal quantity of the specimen of mixed urine from the bladder. Octahedra of calcium oxalate are visible.

The *urine from the right ureter*. This urine contains only a faint trace of albumen, much less even than the specimen of urine from the bladder, and very much less than the urine from the left ureter. Microscopically, a few granular and hyaline casts are present, also a few red blood corpuscles and flat and tailed epithelial cells. Only an occasional leucocyte can be made out.

In view of the results of this examination, showing clearly the presence of chronic nephritis on the right side, a condition admittedly rendering any operation extremely hazardous, together with the fact that the patient, though engaged in active business, suffered no severe pain while using ordinary care, a policy of non-interference with the left kidney, which was evidently the seat of stone, was advised.

Case 3.—Mr. H., æt. 30, was seen in consultation with Dr. N. R. Gorter, of this city, in regard to the advisability of catheterizing the ureters. The abdomen was the seat of an enormous tumor, the swelling, whose limits could be easily defined, extending from the margin of the ribs on the right side to within a finger's-breadth of Poupart's ligaments, two fingers'-breadth below umbilicus in the median line, five fingers'-breadth to the left and two fingers'-breadth above the umbilicus in the median line. Its borders were smooth and rounded. On deep expiration a muffled tympanitic note could be elicited between liver and tumor; dullness just below the margin of the ribs. Behind, the dullness extended to the spine; the area of tumor dullness was not materially changed by position. There had been no history of a sudden diminution of the swelling with the discharge of a large quantity of urine. The patient first noticed the swelling two years ago, since which time it has been gradually increasing in size. The urine, which was of an acid reaction, contained a considerable amount of pus. It had been examined for tubercule bacilli, but with negative results. The family history was unimportant. The patient's general condition was poor, his limbs wasted, skin sallow, appetite poor, intellect dull; over the swelling the subcutaneous veins were considerably dilated.

On November 19th both ureters were easily catheterized under chloroform and several cc. of urine collected from each. The following is Dr. Barker's report of the examination of these specimens:

Specimen No. 1, *from the left ureter* (Mr. H.)—The urine is of a pale straw color and shows a slight whitish sediment. It contains albumen, though in very small quantity. Microscopically, a few leucocytes with polymorphous nuclei, and a few squamous epithelial cells are found to be present in the sediment. The epithelial cells look fresh, and five or six of them are seen occasionally together.

Specimen No. 2, *from the right ureter*.—The urine is yellowish in color, turbid, and yields considerable sediment on standing. It gives a copious precipitate of coagulated albumen on boiling, though not enough to render the urine solid. Still, there are many times as much albumen in this specimen as in specimen No. 1. On microscopic examination the sediment is found to consist largely of pus cells with polymorphous nuclei. There are also many small mononuclear cells, epithelial cells and red blood corpuscles present.

PROCEEDINGS OF SOCIETIES.

THE JOHNS HOPKINS HOSPITAL MEDICAL SOCIETY.

Meeting of December 17, 1894.

DR. J. J. ABEL in the Chair.

The Visual Field as a Factor in General Diagnosis.—DR. DE SCHWEINITZ.

After a description of the normal visual field for form and color; the different characteristics of the results of perimetric measurements obtained when the patient is standing, sitting, or reclining; the phenomena of adaptation of the retina and the changes which varying degrees of illumination produce in the field of vision; the area between the point upon the perimetric semicircle at which a color is recognized as such, and the point where the object is first seen coming in from the periphery (Hering's zone), and the extent of the color field according to the size, saturation and intensity of the test object, Dr. de Schweinitz divided his subject into:

1. The value of the visual field as a means of differentiating between organic and so-called functional affections of the nervous system, particularly hysteria.

In this connection particular attention was directed to the phenomena of *reversal of the color lines;* the possibilities of the hysteric visual field, according to the studies of Dr. de Schweinitz and Dr. John K. Mitchell, being

(*a*) Simple contraction of the color fields with unaffected form fields.

(*b*) Contraction of both form and color fields, the green field being relatively more contracted than the others.

(*c*) Partial or complete reversal of the normal sequence in which the colors are appreciated, most commonly that variety in which the red field is greatest in extent. Under these circumstances the color fields may be normal in size, sometimes even wider than normal, or there may be an associated contraction of all the color fields.

(*d*) Unusual obscurations of the visual field, for example, in the form of hemianopsia, or greater contraction of the fields on one side than on the other, the greater contraction usually being found on the same side with the anæsthesia.*

The practical application of these facts was illustrated by the clinical histories of cases and an exhibition of characteristic maps of the visual-field-alterations.

The value of this method of examination was urged in the study of the implantation of hysteria on an organic lesion, *e. g.* spastic paraplegia of organic origin and hysteria, or of toxic hysteria, *e. g.* hysterical lead paralysis, and was especially commended in the differential diagnosis between true hysteria and so-called hysterical insular sclerosis.*

Finally, the diagnostic import of disturbance of the color-sense in the difficult distinction between certain types of neurasthenic and hysteric patients was pointed out.†

2. The value of the visual field in the localization of intracranial lesions, with special reference to hemianopsia, and the representation of certain retinal areas in the cerebral cortex.

Dr. de Schweinitz rapidly sketched the visual pathway from the peripheral percipient elements in the retina to the cortex of the occipital lobe, and described the results upon the visual field of lesions variously placed in its course, especially referring to the different varieties of hemianopsia and the value of the hemiopic pupillary inaction sign as a differential diagnostic point between lesions anterior and posterior to the primary optic centres.

Hemianopsia of the homonymous variety without pupillary defects, and the localizing value of this phenomenon associated with other symptoms, for example, aphasia, hemiplegia, hemianæsthesia, etc., was briefly discussed, the following features connected with hemianopsia being especially dwelt upon:

(1) Certain hemianopsias indicate that there is a correlation between the parts of the retina and the occipital lobe, and that the removal or destruction of certain portions of the occipital lobe result in loss of certain portions of the visual field.

(2) The probable existence of a centre for the macula lutea in the occipital lobe was described, and the description illustrated by a case of double hemianopsia studied by the author in connection with Dr. Dunn, of West Chester, in which a small central portion of the field of vision surrounding the fixing point remained unaffected.

(3) It was pointed out that the facts just related and illustrated with diagrams indicated that a lesion of this macular centre would manifest itself in the form of a central scotoma, and the author urged a more careful examination of visual fields, and more thoughtful consideration of certain scotomas for which no real cause could be found in the optic nerve itself.

(4) The sense of sight being composed of color, form and light, it was pointed out how the evidence indicated that these

*Consult: A Further Study of Hysterical Cases and their Fields of Vision. By John K. Mitchell, M. D., and G. E. de Schweinitz, M. D. Journal of Nervous and Mental Diseases, January, 1894.

*Consult Buzzard: British Medical Journal, October 7, 1893.

†Consult: Zur Kentniss der Augensymptome bei Neurosen, von L. v. Frankl-Hockwart and Alfred Topolanski. Beiträge zur Augenheilkunde, XI. Heft 1893; and Mitchell and de Schweinitz, *loc. cit.*

centres were situated in the cortex of the occipital lobe, or perhaps the posterior end of the occipito-temporal convolution, and how a relative hemianopsia, that is one, for example, in which the light-sense was preserved, but either the form-sense or the color-sense was wanting, could only result from a cortical lesion.

The subject of *hemiachromatopsia* was illustrated by the description of two cases clinically observed, probably the result of disease of the occipital cortex affecting only the centre for color; although the case of Samelsohn was referred to, in which a glioma-sarcoma pressing upon the optic tract, optic thalamus and corpora quadrigemina was associated with this symptom.

(5) The lecturer called attention to the fact that hemianopsia, although usually a direct symptom, may sometimes be an indirect sign of disease, as the result of what the Germans called *fernwirkung*, which Mr. Swanzy has translated into "distant symptom."

(6) Finally, it was pointed out that hemianopsia may accurately localize a lesion within the cranium when that lesion is itself insignificant and unimportant, the main and essential disease being quiescent. This point was illustrated by the description of a case of large tumor occupying the second and third temporal, and encroaching upon the fourth convolution, which gave no localizing sign of its presence, while a lesion in the cuneal region (a small cyst) was inferred from the study of the case, chiefly the visual phenomena, and laid bare by the surgeon's trephine.*

Catheterization of the Ureters in the Male.—Dr. Brown. [See page 12.]

Dr. Welch.—I should like to have been able to discuss Dr. de Schweinitz's paper. I can only say that I have been fascinated by it. It will make a good many of us go home and read about this subject, and that is a sign of a good paper.

A word with reference to Dr. Brown's important communication. It is important as illustrating the practical value of the application of this procedure. I recall a case in New York in which I made an autopsy—a case in which a serious mistake was made which would have been avoided had this method of determining the presence or absence of the kidneys been used. The patient was a vigorous young German girl who had atresia of the vagina. An effort had been made to reach the uterus by cutting through this closed vagina. They opened the canal up to a certain distance and then abandoned the attempt. Then they found a mythical tumor on the left side. Various diagnoses were made as to the nature of that tumor. The prevailing opinion was that it was connected with the left ovary, and, indeed, that was the opinion of one of the most distinguished surgeons of New York. Dr. Lusk, who saw the case, made a correct diagnosis of movable kidney. The case was operated upon before the class at Bellevue Hospital and the kidney removed. There was nothing the matter with the kidney other than that it was movable. The kidney was brought at once over to my laboratory. It was a very large, succulent kidney. I happened to have made an autopsy a few days before on a man who had only one kidney, and the appearance of the kidney was impressed upon my mind; the thick cortex and the beautiful markings of the cortex, the normal structure greatly exaggerated but perfectly healthy. This kidney looked so much like the one just mentioned that I surmised at once that it was the only kidney the patient had and suggested that, to the horror of the surgeon. The patient lived eleven or thirteen days, voiding no urine. For seven or eight days there were no symptoms to occasion alarm. During the last forty-eight hours uræmic symptoms manifested themselves and the patient died. The autopsy showed that the patient had but one kidney and that had been removed by the surgeon. The operator was very frank in bringing the case to the notice of the medical profession and published it in all its details in one of the medical journals in 1881 or 1882. He discussed at that time all the methods that his ingenuity could suggest as to the possibility of recognizing the presence of a second kidney. I do not know that he at that time even thought of the possibility of catheterizing the ureter. I remember that he discussed the advisability of pressing on the ureter on one side and determining in that way whether the other was present. This is simply one case which shows that there is a practical use for this procedure.

*The case is fully described by Dr. H. C. Wood, under whose care the patient was, in the University Medical Magazine, Volume 1, page 383.

NOTES ON NEW BOOKS.

Napoleone; una Pagina Storico-Psicologica del Genio. By Dr. A. Tebaldi, Professor of Psychiatry, University of Padua. (*Padova*, 1895.)

In this historico-psychological study of Napoleon, Dr. Tebaldi has given the medical profession a most valuable and interesting contribution to Napoleonic literature. The material for the study has been mainly drawn from the previous works of Thiers, Taine, La Bourienne, Yung, Metternich, Levy, Baron de Meneval, Automarchi, Lombroso, and the correspondence of Napoleon.

The point of view of the work is stated in the preface, and closely adhered to in the body of the text: "It is not my purpose to pass judgment on the leader, the statesman or the legislator (for which indeed I feel myself incompetent), but I shall seek only the man, having as foundation for my study the examination of his physical development and inherited tendencies to bear out a psychological law."

The brochure is divided into five parts: Napoleon's Ancestry, Physical Development, Intellect, Personality, and Character, and each theme is treated lucidly and impartially. After these comes a *resumé* of the conclusions reached, which is in some measure the most interesting part of this instructive book.

In the chapter on Physical Development, the question as to Napoleon's being an epileptic is fully discussed, and the writer decides that "the physical examination of Napoleon shows him to be an undoubtedly neurotic individual, not improbably of an epileptic character, and with a constitution which rendered him liable to degenerative diseases, foreshadowing a brief course of life, owing to his hereditary predisposition to cancer."

The study on Intellect emphasizes strongly Napoleon's marvelous rapidity of conception and execution, his absolutely independent judgment, his wonderful power of passing immediately from one difficult subject to another, and gives his own explanation of this ability, when, having compared his brain to a combination of little chests, he says, "When I leave one subject I close that box, to open another if anything else has to be considered." It mentions

his apparently endless endurance, his power of protracted concentration of thought, and while frankly admitting his ignorance of arts and sciences, adds: "In comparison with the warrior, it is true that the man of culture and the legislator dwindles, but it seems to me that some have tried to belittle him too much. It would be absurd to regard Napoleon as a litterateur or a scientist, but to my mind it is incorrect to think that his culture was unequal to the qualities of a great leader."

In treating of Napoleon's Personality full justice is done to his excellence as son and brother, to his frequent generosity to his personal enemies, to his correct views of law and order (for others), to his affection for Josephine; while his impatience of personal restraint, his intense egotism, his absolutely elastic conscience, and his unapproachability as an equal, are frankly admitted.

The tracing by the writer of the change in his character from the First Consul, "pensive, polite, prudent in speech and serious in action"; to General, "enthusiastic, ardent, with '*La Patrie*' on his lips and love in his heart"; and then to Emperor, a man "grown stouter in body and stiffer in mind, cold, proud, preoccupied with ceremonies and etiquette," is excellent.

Some of the salient points in the study on Character are his firmness of will and promptness of action, which are shown to have involved him in troubles more than once when a subordinate officer. His intense capability for anger, of which Thiers says that the flashes thereof "when real lasted but a second, when feigned, as long as needful," and above all, his power to inspire his soldiers with such intense love for himself that as one man they were ready to do and dare whatever he ordered, are forcibly brought out. His alleged superstition is treated as a "pose" whereby he was enabled to mould more to his liking his subjects' opinions with regard to his own destiny.

The analysis of the entire subject throughout the brochure is keen and unprejudiced, the style markedly clear and direct, the author always going straight to the point, and the amount of information and compact reasoning he has succeeded in condensing into the one hundred and sixty pages of the work is unusual. G. H. S.

A Monograph on Diseases of the Breast. By W. ROGER WILLIAMS. (*John Bale & Sons, 87 Great Litchfield St., London.* 1894.)

Mr. Williams, very wisely we think, begins his work on diseases of the breast with a short but clear and concise account of the ontogeny and phylogeny of the mammary gland. The history of the development of the gland in the human being and in the lower mammalia is not only interesting, but gives much aid in understanding the various congenital defects and anomalies in the gland. The second chapter on the morphology of the gland and its secretory functions is generally satisfactory. The author draws attention in this chapter to the common error of supposing that the gland is circular; he points out that it really has a tricuspid form, and that in the ordinary operation for the removal of the breast, the apices of the cusps are nearly always left behind to orignate anew the disease.

The third and fourth chapters deal with mammary variations from defect, and with supernumerary mammary structures; the account of these latter is especially full and clear. Chapter five deals with the various forms of mammary hypertrophy; the subject is well classified and ably handled. The chapter on histology and neoplastic pathogeny is very satisfactory. One point is noticeable in reading this chapter, and this is that our knowledge of the nerve supply of the breast could be considerably extended.

Chapter seven is a statistical one, dealing with the varieties of mammary neoplasms and their relative frequency. The chapter brings out the interesting fact that whilst the relative liability of the female breast to malignant disease is above the average, its liability to non-malignant neoplasms is considerably below the average.

In the chapter on the parasitic theory of cancer we are glad to see that the author discountenances protozoa as a cause of the disease. Most of the prominent pathologists, we think, are now inclined to regard the so-called protozoa as the products of endogenous cell formation. The author states his belief in the non-contagiousness of cancer in this chapter, regarding the apparent cases as merely remarkable coincidences. Chapters eleven to seventeen are taken up with the study of mammary cancer, the term cancer being restricted to neoplasms of epithelial origin.

The chapters on the general morphology and general pathology of the disease are excellent.

The chapter on the operative treatment is not so satisfactory. While the author recommends a fairly radical operation, we cannot agree with him that the mere skinning off the fascia of the great pectoral muscle is sufficient in most cases. Whilst the well known researches of Heidenhain and others have shown that in most cases the progress of the disease is arrested for some time by the pectoral fascia, yet actual results have shown that entire removal of at least the sternal part of the pectoralis major has resulted more favorably to the patient.

In the chapter on the pathology of breast cancer we are glad to see that Mr. Williams has separated so sharply the intra-canalicular fibroma, a non-malignant disease, from the tubular form of cancer, which is malignant; the two are usually confounded.

In the chapter on cancer of the male breast one is struck with the fact that the disease is much more frequent than one would suppose from reading most text-books. According to the author, carcinoma occurs in the male breast once where it occurs in the female 99 times.

Chapters 17 to 23 take up sarcoma and non-malignant tumors. These chapters show the same care and systematic arrangement as those on cancer.

The closing chapters on mastitis, tubercle and syphilis of the breast are highly satisfactory.

Taking the work as a whole, one is impressed with the extremely systematic manner in which it has been arranged. The language is clear and forcible, and the references are profuse and culled from the best sources. The book is well printed upon good paper.

We would recommend Mr. Williams' work to any one who wishes to acquire a clear understanding of diseases of the breast without wasting unnecessary time on superfluous theories.

BOOKS RECEIVED.

A Manual of Organic Materia Medica and Pharmacognosy. An introduction to the study of the vegetable kingdom and the vegetable and animal drugs, etc. By L. E. Sayre. With 543 illustrations. 1895. 8vo, 555 pages. P. Blakiston, Son & Co., Philadelphia.

On Chorea and Choreiform Affections. By William Osler, M. D. 8vo. 1894. 125 pages. P. Blakiston, Son & Co., Philadelphia.

Saint Thomas's Hospital Reports. Edited by Dr. T. D. Acland and Mr. B. Pitts. New Series, vol. xxii. 8vo. 1894. J. & A. Churchill, London.

Real-Encyclopädie der gesammten Heilkunde. Medicinisch-Chirurgisches Handwörterbuch für praktische Aertze. By Prof. Dr. Albert Eulenberg. Third thoroughly revised edition. Vol. iv. Brenzcatechin—Cnicin. 1894. Urban & Schwarzenberg, Vienna and Leipsic.

Transactions of the Congress of American Physicians and Surgeons. Third Triennial Session held at Washington, D. C., May 29, 30, 31, June 1, 1894. Published by the Congress. New Haven, Conn.

Travaux d'Electrotherapie Gynecologique. Archives semestrielles d'electrotherapie gynecologique. By G. Apostoli. 1894. Société d'Editions Scientifiques, Paris.

Anatomy and Art. The Annual Address read before the Philosophical Society of Washington, December 12, 1894. By Robert Fletcher, M. D. 1895. Judd & Detweiler, Washington.

On General Paralysis, with a critical digest. By W. Julius Mickle, M. D., F. R. C. P. (London), Grove Hall Asylum, London. Reprint from Brain. 1894. Macmillan & Co., London.

Practical Suggestions respecting the Ventilation of Buildings. By John H. Kellogg, M. D. Reprinted from the 19th Annual Report of the Michigan State Board of Health. 1891. Lansing, Mich.

The Relation of Static Disturbances of the Abdominal Viscera to the Displacements of the Pelvic Organs. By John H. Kellogg, M. D. Reprinted from the Proceedings of the International Periodical Congress of Gynecology and Obstetrics. 1894. Modern Medicine Publishing Company, Battle Creek, Mich.

The Rain Bath at the Utica State Hospital. By Wm. Paul Gerhard, C. E. 1894. Republished from the Engineering Record. Privately printed by the Author. New York.

Traumatic Paralysis of the Abducens Nerve. By Dr. O. Purtscher, of Klangenfurt. Translated by Dr. Harry Friedenwald, of Baltimore. Reprinted from the Archives of Ophthalmology. 1894.

Traumatic Paralysis of the Abducens Nerve. By Dr. Harry Friedenwald, of Baltimore. Reprinted from the Archives of Ophthalmology. 1894.

The Effects of Various Metals on the Growth of Certain Bacteria. Read before the Association of American Physicians, May 30, 1894. By Meade Bolton, M. D., Johns Hopkins University. Reprinted from the International Medical Magazine for December, 1894.

Annali dell' Istituto d'Igieni Sperimentale della R. Università di Roma. Edited by Prof. Angelo Celli. Vol. iv., Fasciculi 2 and 3. 1894. Ermanus Loescher & Co., Rome.

Bulletino della Reale Accademia Medica di Roma. Edited by Prof. G. Colasanti and Prof. G. Sergi. 1894. Tipografia Innocenzo Artero Rome.

CLIMATOLOGY AND PUBLIC HEALTH.

The following announcement is made by the U. S. Weather Bureau:

Washington, D. C., January 2, 1895.

The interest manifested by every class of people in the subject of climate and its influence on health and disease has determined the Honorable the Secretary of Agriculture, through the medium of the Weather Bureau, to undertake the systematic investigation of the subject.

It is hoped to make the proposed investigation of interest and value to all, but especially to the medical and sanitary professions, and to the large number of persons who seek, by visitation of health resorts and change of climate, either to restore health or prolong lives incurably affected or to ward off threatened disease.

The study of the climates of the country in connection with the indigenous diseases should be of material service to every community, in showing to what degree local climatic peculiarities may favor or combat the development of the different diseases, and by suggesting, in many instances, supplementary sanitary precautions; also by indicating to what parts of the country invalids and health-seekers may be sent to find climatic surroundings best adapted to the alleviation or cure of their particular cases.

The hearty co-operation of the various boards of health, public sanitary authorities, sanitary associations and societies, and of physicians who may feel an interest in the work, is asked to achieve and perfect the aims of this investigation.

No compensation can be offered for this co-operation other than to send, free of cost, the publications of the Bureau bearing upon the climatology and its relation to health and disease, to all those who aid in the work.

Co-operation will consist in sending to this office reports of vital statistics from the various localities. That these reports may be of value, it is evident to all that they should be accurate and complete, and be rendered promptly and regularly. Blank forms of reports have been prepared so as to occasion as little trouble and labor as possible on the part of the reporter, and will be furnished by the Bureau on application.

At the very beginning of the investigation it is not possible to outline precisely the channels through which the results obtained will be made public, but it is hoped to publish soon a periodical devoted to climatology and its relations to health and disease. The publication will probably resemble in size and general appearance the present Monthly Weather Review, the subject-matter being, of course, different.

More detailed information will be furnished on application.

MARK W. HARRINGTON, *Chief of Bureau.*

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Report in Surgery, II.

The Results of Operations for the Cure of Cancer of the Breast, performed at the Johns Hopkins Hospital from June, 1889, to January, 1894. By WM. HALSTED, M. D. Price $1.00.

Report in Gynecology, III (in press).

By T. S. CULLEN, M. B.

I. Hydrosalpinx, its Surgical and Pathological Aspects, with a report of twenty-seven cases.
II. Post-operative Septic Peritonitis.

Report in Dermatology, I.

Containing Protozoic Dermatitis, Molluscum Fibrosum, Urticaria, etc. By T. C. GILCHRIST, M. R. C. S. (To appear in January.)

Price of Volume IV complete, bound in cloth, $5.00.

Volume V.

Nos. 1, 2, 3.—Report on Malaria. By WM. OSLER, M. D.; W. S. THAYER, M. D.; J. HEWETSON, M. D.; L. F. BARKER, M. B. (To appear in February.)

THE JOHNS HOPKINS HOSPITAL BULLETIN, VOLUME VI.

The BULLETIN of the Johns Hopkins Hospital enters upon its sixth volume, January 1, 1895. It will contain original communications relating to medical, surgical and gynecological topics, reports of dispensary practice, reports from the pathological, anatomical, physiologico-chemical, pharmacological and clinical laboratories, abstracts of papers read before, and of discussions in the various societies connected with the Hospital, reports of lectures and other matters of general interest in the work of the Johns Hopkins Hospital and the Johns Hopkins Medical School.

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